

## Periscope.

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### a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

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THE NERVES OF THE LABYRINTH.—Horbaczewski, in a communication made to the Academy of Vienna, gives the results of his investigations on the nerves of the internal ear in man and animals. He finds that in the sheep the nervus vestibuli and the nervus cochleæ are completely separated from each other at their origin. The primitive fibres of the cochlea remain always much finer than those of the vestibular nerve. It is remarkable that the size of the vestibular nerve increases with the size of the animal in a much more rapid ratio than the size of the cochlear nerve, so that, for example, the nervus cochleæ of the horse appears much more slender, as compared with the nervus vestibuli, than that of the rabbit or of man. In the sheep the nervus cochleæ is distributed exclusively to the cochlea, and the nervus vestibuli to the remaining portion of the internal ear. But this distinction is by no means so sharply marked either in man or in the horse. In the horse there is constantly an exchange of fibres, though a comparatively slight one, between the two nerves. In man the two roots form a common trunk, which subsequently divides into the two nerves. The nervus cochleæ is not, however, exclusively distributed to the cochlea, but gives off a fine branch, which runs in the recessus cochlearis to the vestibular extremity of the ductus cochlearis, and through the macula cribrosa to the septum of the two sacculi contained in the vestibule. He thus demonstrates the correctness of the statement made by Flourens, that the vestibular nerve is a completely distinct pair from the auditory nerve, or nervus cochleæ. *Lancet* (Am. Rep.) July.

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THE SEGMENTATION OF THE HEAD.—By slow degrees an approach is being made to a true understanding of this most difficult and interesting question. The old explanations by archetypes and by the structure of the highest developed skulls, have fallen into disfavor. Attempts to settle the cranial segments by considering the distribution of nerves in the adult have been shown to be unsafe, because nerves are necessarily adaptational in their character, and liable to the greatest modification on changes taking place in the organs they supply. The development of nerves, however, is a much surer guide, showing primitive and funda-

mental characters. The nerves behind the ear are five in fishes, although the number of strands of which the vagus is made up in some cases points to a loss of distinct nerves and segments in the hinder part of the head. The auditory and facial nerves originate as one, so that the auditory appears as a specialized portion of the facial. The trigeminal likewise arises as a single nerve, and in front of this there is no nerve having a similar history to these and the spinal nerves. Thus we have an indication of seven segmental nerves issuing from the brain case. When the visceral clefts are considered, we find in sharks six clefts indicating seven segments, or one more, if the mouth be regarded as a cleft. The head-cavities between the outer wall of the head and the mucous membrane of the throat, discovered by Mr. Balfour in sharks, furnish a similar number. They are eight in all, one premandibular, one mandibular, one hyoid, and five branchial. Thus the examination of three sets of organs leads to the assignment of eight body segments to the head. But the question is far from being settled so long as the brain-case itself and the brain cannot be satisfactorily explained. *Nature*, Aug. 9.

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THE MODE OF CONTRACTION OF MUSCLES.—At the session of the Soc. de Biologie, July 6, (rep. in *Gaz. des Hopitaux*), M. Onimus presented, in the name of M. Trouvé, an electric apparatus designed to show the mode of muscular contraction.

"M. Trouvé, struck by the considerable effects produced on the muscles by a feeble electric current, thought that here ought to be found one of the principal receivers of the electro-motive force. Directing his experiments with this idea, the result was the construction of an apparatus that answered to all the functions of muscle.

"M. Trouvé compared the active molecules of the muscle to little electro-magnets, attracting each other by their opposite poles. It is easy to understand how such a mechanism acts. The action of two electro-magnets, multiplied by the square of the section, gives a fair idea of the work done by the system, and of the amplitude of the movement, but it does not account for the very considerable effects observed in the muscle. Therefore, M. Trouvé, continuing his researches, obtained the proof that it was necessary to totalize each individual effort of the electro-magnets, since this total ought to give mathematically the result of the whole power of the mechanism, and correspondingly a better idea of that of the muscles.

"What mechanism could now sum up thus these effects? M. Trouvé, remembering the child's toy, consisting of articulated parallelograms on which toy soldiers are arranged and moved, constructed an apparatus composed of a series of electro-magnets attracting each other by their opposite poles, and connected by articulated parallelograms which totalize all their actions.

"Without presuming to reproduce in any way the form of the muscle or pretending to imitate all its movements, this little apparatus never-

theless explains nearly all its properties, and permits us to formulate the following theory.

"The power of the muscle is the resultant of all the molecular attractions. This little apparatus explains in a very satisfactory fashion the total contraction of a muscle by localized electrization (method of Duchenne de Boulogne), without having recourse to reflex actions or the propagation of molecular connection. It permits, also, an explanation of the persistence of muscular contraction in its effects by the persistence of magnetism."

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DISSIMILAR PROPERTIES OF TACTILE AND ELECTRIC SENSATIONS.—We take the following from the report in the *Gaz. des Hopitaux* of the proceedings of the Soc. de Biologie, July 28th, 1877.

M. Bloch had, in May, 1875, submitted to the Soc. de Biologie certain experiments which he had performed to test the rapidity of the sensory nerve current in man.

"These experiments," said he, "were based upon the sensations of tact, and the excitation employed consisted in mechanical shocks given by a flexible index fixed in a fly-wheel, and striking at each turn the part upon which he was experimenting.

"I have undertaken these experiments again, but substituted an electric shock for the mechanical one formerly employed.

"My first care was to determine the duration of the persistence of the electrical sensations, and, in seeking this element of the problem, I have met with results altogether unexpected, which form the subject of the present communication.

"The electric shocks were given by two breakings of the current, by means of two induction coils.

"The closing and opening of the current are made upon one of Mareys polygraphs, covered with paper and carrying a smooth, narrow metallic band, over which two rubbers are made to pass successively. These may be separated to a greater or lesser extent at will.

1. In receiving the two shocks at the same point, say on a finger, they will be fused, and there will be a single sensation for the interval of 1-31 second.

"This figure approaches very nearly those given by many physiologists. If it is a little smaller than that of others, if it consequently proves more delicacy and a distinction of the two electric shocks pushed farther, this holds good especially when I receive two shocks only, and not a continued succession of them. In this latter case the sensibility shows itself very quickly, and the fusion of sensations takes place with still greater intervals.

2. "If one of the shocks is received on the index and the other on the median finger, we feel them perfectly distinct at the interval of 1-31 of a second, and the combined sensation appears only when they are 1-43 of a second separated.

3. "With the index and the auricular of the same hand, we distinguish

the first from the second shock very clearly at 1.43 of a second. It is necessary to decrease the interval between them to 1.62 of a second in order to produce the fusion of the two sensations and not to be able to recognize the first from the second.

4. "With the two hands, at 1.62 of a second, the order of the shocks is easily perceived, even without knowing it beforehand. The synchronism of the two sensations appears only at 1.83 of a second. For the two feet the limit is the same, 1.83 of a second. Thus in this latter case the persistence of the first sensation endures no longer than when we try the two hands, the reverse of what we observe with mechanical shocks. This last I demonstrated in my researches in 1875.

5. "In the case of the mechanical shocks, which I have only studied only in parts quite distant from one another, I found that the interval required to obtain synchronism is always the same, 1.45 of a second, whether we use the two hands, or the thumb and the auricular of one hand, or the thumb and index of the same, and that the persistence of the first sensation is rather a constant fact.

"The sensation of tact and the electric sensation have, therefore, quite different peculiarities as regards the question before us.

"Another corroborating fact. While the distinction of the two electric shocks is difficult when they strike the same point, since the fusion takes place at 1.31 of a second, inversely, the same part struck mechanically with two blows, they are dissociated at least as well as when at two neighboring points, and perhaps even with greater delicacy."

THE EFFECTS OF MECHANICAL, CHEMICAL, AND ELECTRIC EXCITATION OF THE PNEUMOGASTRIC.—MM. Dastre and Morat communicated to the Soc. de Biologie Aug. 11 (rep. in *Le Progres Médical*) a note on the effects of mechanical, chemical, and electric excitations of the pneumogastric in the tortoise. The arrest of the heart from excitation of the pneumogastric, as is well known, does not occur except when this excitation is made by the use of electricity, the current frequently interrupted and of a certain intensity. MM. Dastre and Morat, operating on a suitable animal, the tortoise, have obtained arrest of the heart by the habitual excitants of the nerves; the result is such as to relieve the doubts that have been produced relative to the *role* of the pneumogastric as a nerve of arrest. The following are the conclusions of these authors:

1. The pneumogastric of the tortoise is put in action under the influence of mechanical excitants, (ligature, tearing, crushing, cutting) and under the influence of chemical excitants, (glycerine); 2, the arrest of the heart is also obtained by the use of *very feeble* currents (tetanizing) hardly sensible to the tongue; 3, the arrest of the heart is also obtained by the single discharge of induction, by the opening or closing of a strong current. The effect of the opening is always greater than that of the closing of the current. These results were very neatly observed during the summer season with a temperature of 20° to 25° c. (= 68° to 95° F.) The pneumogastric being usually considered as producing the arrest only under the

influence of a very strong tetanizing electric current, some physiologists have been led to consider it as only an ordinary motor nerve, a strong excitation of which caused the paralysis and stopped the heart, while a feeble one accelerated its pulsations. MM. Dastre and Morat show, on the other hand, that the pneumogastric responds in only one way to excitations, no matter what their nature, their intensity and their rhythm. This action is always *arrest in diastole*.

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ELECTRIC VARIATION IN THE CARDIAC MUSCLE.—Muscular contraction it is known, is always accompanied with electric phenomena; the difference of electric potential between two points of a muscle undergoes a diminution, which, according to Bernstein, precedes by about 1-100th of a second the contraction of the muscle. This *electric variation* has been observed on various muscles, and in particular on the heart, (by DuBois Reymond and Kuehne,) and recently M. Marey has represented it graphically by photographing the indications of a Lippmann capillary electrometer. We learn from the *Journal de Physique*, that M. De la Roche has tried the experiment on the heart of a living man. Two points of the epidermis of the chest were connected with the poles of a capillary electrometer, by means of electrodes, formed each of a bar of amalgamated zinc, with a plug of muslin at its lower end saturated with sulphate of zinc. Held with insulating handles, the bars were applied, one with its plug opposite the point of the heart, under the left nipple, and the other to another point of the chest. The mercurial column was then seen to execute a series of very distinct periodical pulsations synchronous with the pulse; each pulsation even marked the double movement of the heart, (of the auricles and ventricles.) The amplitude corresponded to about 1-1000th Daniell. *Nature*, June 14.

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THE NERVES OF THE DURA MATER.—At the session of the Soc. de Biologie, Aug. 4, (rep. in *Le Progres Médical*), M. Duret made a communication, the principal conclusions of which are as follows: 1. The excitation of the nerves of the dura mater produces, by reflex action, convulsive movements of the corresponding side of the body, and sometimes, also, of the opposite side, in the face, the trunk, and the members. These facts were already published by M. Bochefontaine. M. Duret confirmed the observations of his predecessor; but he noticed, further, that if we inject through the hole in the skull a small quantity of coagulable or irritant substance, incapable of acting by compression, we observe a veritable tetanic contraction, a pleurosthotonos of the same side of the body.

2. The nerves of the dura mater may act, by reflex action upon the pupils, dilating them, especially the one on the corresponding side (a fact already noted by M. Bochefontaine). 3. Their *brusque* excitation may produce a veritable respiratory syncope and death from contraction of the diaphragm. 4. They act also by reflex action on the vaso motor

nerves of the eye, and of the brain of the same side, and this action is rapid and powerful. 5. By the same mechanism they may augment the tension of the cephalo-rachidian liquid, and that of the general circulation.

6. It is probable, also, that they act on the secretions, since in the animals subjected to experimentation it is not uncommon to see the occurrence of salivation, micturition and involuntary dejections.

In a clinical point of view M. Duret called attention to the importance of the observations of M. Bochefontaine and himself. Inflammation of the dura mater, as he had shown on a dog in which he had injected iodine under the skull, reveals itself by convulsions, and often by contractures of the *corresponding side*, and sometimes on the opposite side. It will be henceforth possible, therefore, for the surgeon to ascertain whether a bony splinter or a foreign substance acts on the dura mater or on the nervous substance of the hemispheres; for if the dura mater is injured or irritated, the convulsions and the contracture follow on the same side, while if they were produced by encephalitis they would be on the opposite side. The state of the pupil, of the globe of the eye, of the respiration, of the circulation, etc., will permit us to assure ourselves of the diagnosis. It is not a mere supposition, guided by the existence of these signs a distinguished physician of Bernay, formerly *interne* of the hospitals, Dr. Blain, suspected in a wounded man suffering from fracture of the skull with depression, a splinter irritating the dura mater. He performed the operation of trephining, and saved the patient.

THE FUNCTION OF LANGUAGE.—At the session of the Acad. de Médecine of Paris, July 23, M. Edward Fournié, physician at the Deaf and Dumb Institute, read a communication, which is reported as follows in the *Progres Médical*.

In the preliminaries of this memoir the author repudiated the introduction of philosophical systems into physiological studies, and he shows that the psychological method, resting simply upon each one's own feelings, is an unreliable and incomplete instrument in the hands of the philosophers, when they pretend to study the physiology of the brain under the name of psychology. Nevertheless, as the determination of the psychic elements is a necessary preliminary in every study of the brain, M. Fournié recognizes the necessity of the use of the psychological method, on condition that it is employed in concert with pathological anatomy, experimentation, and under the direction of a physiological analysis.

After these preliminaries, M. Fournié examines, critically, the prevalent doctrine of the localization of the function of speech in the third left frontal convolution. This, as every one is aware, is the doctrine of MM. Bouillaud and Broca. The author accepts all the facts of pathological anatomy on which this doctrine is supported, but he proposes to prove that these facts are not properly open to the interpretation given them: 1. Physiological analysis of intelligent movements, of which speech is the

highest type, informs us that in the execution of these movements, phenomena of sensibility, of memory, under the control of the judgment and the will, and phenomena of movement directed by the special senses of sight or hearing, enter as necessary elements of the case. But it is impossible to admit that all these activities, which we find, moreover, in every cerebral functional activity, are localized in the third left frontal convolution. 2. Anatomy informs us that in all points of its extent, the nervous system is subject to laws of organic symmetry, and consequently to laws of functional symmetry.

The loss of speech from a lesion of only one side of the brain does not prove speech is localized on that side; it proves that the two sides are absolutely indispensable for the formation of speech. In support of this opinion, M. Fournié shows that if in the phenomena of sensibility and of memory the two hemispheres can act for each other, it is not so for the excito-motor phenomena, which have an analagous *role* on the two sides, but distinct as to the locality of the result obtained. To complete these demonstrations, M. Fournié submitted the formation of the word to a very delicate analysis; he considered it first as a sensory phenomena, then as a phenomenon of movement, and closed with the following conclusions: 1. The material conditions of speech considered as a sensory phenomena, are found in either hemisphere; 2. The material conditions of speech considered as a motor phenomenon, are indispensably connected with both hemispheres; 3. Contrary to the opinion of MM. Broca and Bouillaud, it is not possible to admit that the material conditions of speech are localized in the third left frontal convolution, notwithstanding the exactness and the authenticity of the facts of pathological anatomy on which this view is supported.

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**VASCULO-CARDIAC REFLEXES OF SENSORIAL ORIGIN.**—At the session of the Soc. de Biologie, June 30, (rep. in *Gaz. des Hopitaux*), M. Charpentier made, in his own name and that of M. Couty, a second communication on the mechanism of the vasculo-cardiac reflexes of sensorial origin.

These reflexes are essentially variable as regards their form and intensity, with certain individual or experimental conditions, and in the same animal, with the duration, the intensity of action, and the repetition, but not with the nature of the external excitant.

The variations of tension are independent of the cardiac troubles; these cardiac disorders are transmitted solely by the pneumogastriacs.

The integrity of the brain is indispensable for the production of vasculo-cardiac sensorial reflexes; and *en resumé* it is not the sensation itself that causes these reflexes, but a more remote inconstant cerebral action of an emotional nature, thus acting through the intermediation of the mesencephalon.

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**CEREBRAL THERMOMETRY.**—At the session of the French Association for the Advancement of Science, Aug. 20, (rep. in *Le Progrès Médical*) M.

Broca read a remarkable communication on cerebral thermometry and the *role* it might play in the diagnosis of diseases of the brain. To obtain this temperature he used a finely graduated thermometer, one face of the bulb applied against the cranium and the other enclosed in a sack enveloped in layers of wadding. The external temperature, therefore, could not affect the mercury. Generally M. Broca employed six of these appliances arranged in a sort of crown around the head. In this way he obtained the temperature at six different symmetrically disposed points on the circumference of the cranium; the two anterior thermometers placed directly behind the external orbital apophyses, the two middle ones above the ears in the temporal region, and the posterior ones in the occipital region. For the sake of brevity, he gives a name to each of these thermometers; those of the left side he denominates F., (frontal) T. (temporal) and O., (occipital); those of the right side correspondingly, F', T'. and O'. When we add together the figures given by these thermometers and divide the sum by six, we have the mean temperature of the head; but each thermometer gives only the temperature of the point to which it is applied, and we can compare this figure with that of the others. This comparison is the one thing that gives us important results.

The experiments of M. Broca were begun in 1869, but it is especially since 1873 that he has applied his researches to the diagnosis of cerebral affections. In order to have a basis of observation it is needful to know, first, the physiological temperature of the various regions of the head. For this purpose he experimented upon the *externes* and the attendants of his service who possessed nearly the same intellectual development, and he endeavored as much as possible to have each of his experiments performed under identical conditions. But here, this difficulty presents itself; the thermometer is, as we have said, applied to the cranium, and is separated in consequence from the substance of the brain by media of various thicknesses. F. and F', situated at the external part of the temporal form, are only separated from the brain by a thin layer of muscle and by the bone itself, which is here not of much thickness. At T. and T'. we do not have the muscle, and the bone is not very thick, but the hair (which, by the way, is removed as much as possible out of the way) is a bad conductor. At O. and O'. we have also the hair, and the thickness of bone is greater than at the temporal fossa. Nevertheless, these sources of error are truly too slight to explain the very noticeable variations of temperature that exist between these different regions.

The averages given were obtained from twelve *externes* and attendants at the Hospital des Cliniques, placed as nearly as possible under the same physiological conditions, the thermometer being left in place in each examination some twenty minutes. The maximum temperature of the brain was found to be  $34^{\circ} 85$  c. ( $= 94^{\circ} 70$  Fahr.), the minimum,  $32^{\circ} 80$  ( $= 91^{\circ} 04$  F.), the mean temperature should, therefore, be  $32^{\circ} 82$ . But if we compare the left thermometers, F., T. and O., to the right ones, F', T'. and O', we notice that, as a regular thing, the left side temperature is higher than that of the right. Thus on the right side the mean temperature was  $33^{\circ} 90$  ( $= 93^{\circ} 09$  F.), while on the left it is a little over  $34^{\circ}$  ( $= 93^{\circ} 20$ ). In



the normal condition, therefore, the temperature of the left side is higher than that of the right, to the amount of about one-tenth of a degree centigrade. But, what is remarkable, this inequality only exists when the brain is in a state of rest. When the brain is at work there is a tendency to establish an equilibrium, and for the two hemispheres to show equivalent figures. It is not necessary to suppose with M. Broca that the left hemisphere is better irrigated than the other, that it receives a greater quantity of blood; but that when the brain is in action, as the right hemisphere is less prepared and less mobile, it is compelled to greater efforts, the call for blood is louder on this side, and tends to produce an equilibrium between the two hemispheres.

M. Broca does not rest here, but having compared the two sides of the brain, he tested the different lobes of the same hemisphere and found that the temperature of the occipital lobe was  $32^{\circ} 92$ , that of the temporal lobe  $33^{\circ} 72$ , and that of the frontal  $35^{\circ} 28$ . It will be seen from these figures how the functional activity of the frontal ought to exceed that of the temporal or occipital lobes.

Such are the results obtained by M. Broca in the brain at rest. In action, however, the figures are different. It was difficult to apply to all subjects an equal test not harder for one than for the other. M. Broca tried that of reading, about equally familiar to all, at least to all medical students. After ten minutes of reading aloud, the mean temperature arose from  $33^{\circ} 82$ , the normal mean, to as high as  $34^{\circ} 23$ . We have here, therefore, a difference of nearly half a degree cent. in favor of the active brain.

The clinical researches of M. Broca are no less important; he has been able to demonstrate by the thermometer a certain sign of cerebral embolism; he has been able to determine the portion of the brain deprived of sanguine irrigation. For a long time already he had shown that curious phenomena of temperature accompanied embolisms in the members. He had shown what, moreover, might be *a priori* expected, that the general temperature of the limb became lowered, but at the point itself where the stoppage took place there was a rise of temperature. In cases of ligation it has been attempted to explain this apparently paradoxical condition by the constriction of the nerves. But this explanation is not valid in cases of spontaneous obliteration, as in embolism, since the nerves cannot be injured by the lesion. M. Broca explains this local rise of temperature by the collateral circulation that is established; the blood no longer passes through the deeper lying vessels, it penetrates and dilates the superficial vessels, and the peripheral circulation being more active, the temperature will be raised. The maximum of temperature will, therefore, be at the horizon of the embolism.

Nothing of this kind can occur in the brain, since the vessels of the collateral circulation are not sufficiently numerous. When cerebral embolism occurs, seven times out of ten the clot reaches the sylvian artery, and either obliterates it completely or one of its trunks. In this case what should we expect to see? The blood, entering no longer into the territory supplied by the sylvian artery, the temperature will be lowered in the cor-

responding thermometer, but the irrigation will be more active in the frontal and occipital lobes, and the mercury will rise in the corresponding thermometers.

Two cases completely bearing out this theory, as far as they went, were given by M. Broca, and the position may be accepted, therefore, that in the region of the embolism there is decrease of the temperature of the brain.

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THE COURSE OF THE FIBRES IN THE NERVE CENTRES.—Flechsig, *Centralblatt*, No. 3, publishes the following as the results of some of his more recent researches.

1. Each pyramid of the medulla (compare my definition given elsewhere), continues, without being interrupted in ganglion cells, through the pons, crus, and inner capsule, into the centrum semióvale of the corresponding hemisphere, and preferably into the tract corresponding to the central convolutions.

2. The continuation of the pyramid fibres forms in the crus and inner capsule, a compact cord, the situation and limits of which are rather satisfactorily established; in the external capsule it has generally an elliptical section, and passes between the lenticular nucleus and the middle third of the thalamus.

3. The tracing of the formation of the medullary sheath furnishes a better topography of the fibres of the inner capsule than was heretofore attainable.

4. In the cerebrum, also, the development history and "secondary degenerations" afford accordant results as to the course of the fibres.

The observations are more fully published in the *Arch. d. Heilkunde*, 1877, 2 fig., under the head, "On 'Systemic Diseases' in the Cord."

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THE ANASTOMOSES OF THE HYPOGLOSSUS. M. Holl, *Zeitschr f. Anat. u. Entwicklungsgesch.* II. S. 82 (Abstr. in *Centralbl. f. d. Med. Wissensch.*)

The anterior branch of the first cervical nerve, after reaching the inner margin of rectus capitis muscle and the anterior half of the vertebral column, and giving a twig to the muscle and anastomosing with the sympathetic, divides into two stems, an upper and a lower one, the first of these passes at a right angle to the hypoglossus muscle and disappears in its sheath; and the other descends and serves for connection with the second cervical nerve; forming generally an arched anastomosis which constitutes the first "ansa cervicalis." The second cervical nerve sends a branch up to the twig from the first entering the hypoglossus sheath, it clings closely to it, courses median-wards, also enters the sheath and runs, more or less visible, down on the convex surface of the muscular nerve of the tongue, breaks through the sheath in its passage over the horizontal stem (after giving out a twig which penetrates the hypoglossus toward the median line), and appears as the descending cervical nerve on the anterior surface of the internal jugular vein. It forms in its further course, through

anastomotic connections with the second and third cervical nerves, the so-called *ansae cervicales*, and together with the said nerves innervates the hyoid group of muscles. It receives no branch from the hypoglossus, and hence the descending cervical is not formed by a descending branch of the hypoglossal and the cervical nerves (HENLE). Frequently the cervical nerve does not enter the sheath of the hypoglossus, instead of this, it rises from the second cervical and accompanies the branch mentioned from the first cervical as far as to its entry into the sheath into which it sends only a minute filament (which is soon absorbed by the hypoglossus fibres). But it does not itself enter, but separated by a perceptible interval it courses parallel with the hypoglossus, approaching it at its greatest convexity, and is connected with it by nerves which starting from the second or third, or from both, ascend to this descending branch, enter into the horizontal portion of the twelfth cranial nerve, run for a little distance enclosed in its neurilemma and are then peripherally distributed to the muscles. It is seen in such preparations how the descending cervical nerve takes its own course aside from the twelfth cranial nerve, and how this branch, on the one hand only high up, and on the other only at the convex margin is connected with it.

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THE MOVEMENTS OF THE BRAIN.—MM. Giacomini and Mosso presented to the Acad. des Sciences in Paris, Jan. 3. (rep. in *Bull. Gen. de Thérapeutique*) the photograph of a woman, thirty-seven years old who, from a syphilitic disease of the cranial bones, had lost a great part of the frontal and the two parietal bones. In order to study the movements of the brain they had fitted the opening in the skull with one of Marey's graphic apparatuses.

They found that the form of each pulsation of the brain varied according to circumstances. It was different from the sphygmographic pulse of one artery, and there was not even a resemblance with traces taken on the fore-arm introduced into a cylinder filled with water and placed conveniently in communication with a Buesson apparatus.

During profound sleep, with snoring, there was a quite pronounced increase in the height of the cerebral pulsations: the respiratory oscillations and undulations became much more pronounced.

Certain causes produce the same change of volume in the brain and in the extremities; others cause variations found simultaneously in opposition in the brain and various parts of the body.

The experiments on the changes of volume of the brain and of the fore-arm of man have furnished the element of a correlative physiology of the blood vessels, and we are enabled to-day to study the modifications produced by identical excitations in the vessels of different parts of the body.

During compressions of the carotids, the cardiac pulsations disappear almost entirely. When the arterial circulation is re-established, the pulsations increase in height, and the brain, after a rapid increase of volume, presents a contraction which continues with the pulsations stronger than before.

By compressing the jugular veins, we cause an increase of volume of the brain. After twenty or thirty seconds of venous congestion of the brain, the volume of this organ begins to decrease. During the nervous congestion the pulsations increase considerably in height, and this increase remains for a considerable time, even after the re-establishment of the normal venous circulation. After the venous congestion we always observe a diminution of the volume of the brain, which is probably produced by a contraction of the vessels. An interruption of the respiratory movements produces the same effect. Very extensive respiratory movements exercise a profound influence upon the form of the cerebral pulsations, and we see in the brain the same phenomena that are produced during compression of the carotids. During the occlusion of the femoral arteries, the cerebral pulsations appear more acute and higher: at the moment when the circulation of the blood is re-established we see a rapid diminution of the height of the pulsations.

Every movement of the body and all intellectual labor, is reflected upon the brain, which undergoes a visible modification in its volume and in the form of its pulsations.

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#### THE MODE OF TERMINATION OF THE NERVES OF TOUCH, IN MAN AND THE VERTEBRATED ANIMALS:

The following is a resume of a lengthy article by Dr. J. G. Ditlevsen, of Copenhagen, published in the *Nordiskt Medicinskt Arkiv*, Vol. VIII., No. 11:

Up to this day, as we know, our experience with regard to the termination of the nerves of touch, has been limited to the knowledge that a portion of these nerves terminate in terminal corpuscles, particularly in the tactile corpuscles of Meissner, and the end-bulbs of Krause. It was admitted that the others terminated either in free extremities, or in closed terminal nets.

The object of the present treatise is to show that recent researches permit, in the opinion of the author, certain views otherwise entirely clear and much more satisfactory, both to the anatomist and the physiologist.

The author then passes in review all the researches, the result of which is the demonstration of the termination of the nerves of touch in terminal cells. These researches are the following: 1st, the observation of Leydig that the nerves of the *poils tactiles*, or feeling bristles (*Varborster*) terminate in special cells, found in the sheaths of the hair roots, an observation, the correctness of which was confirmed by Sertoli, several years later. 2d, the observation of Merkel on the tactile corpuscles in the tongue and beak of birds, corpuscles that in reality are nothing but groups of terminal nerve cells; 3d, the discovery by Leydig of similar tactile corpuscles in the skin of reptiles and amphibia; 4th, the establishment of the fact that in man, also, the corpuscles of Krause are groups of terminal nerve cells, (Longworth and Waldeyer); 5th, the research of Langerhans on the *Amphioxus lanceolatus*; 6th, that of Merkel on the human skin, and on that of mammals and birds; 7th, that of Ditlevsen

on the skin of frogs. All these researches show that the cutaneous nerves terminate in special terminal cells.

If we add the observations, according to which the tactile corpuscles in man and the terminal corpuscles in birds (ordinarily called Pacinian corpuscles), are probably also groups of terminal cells, which other observations are all enumerated in the original treatise, it is likely that the principle traits of the mode of termination of the nerves of touch in man and the other vertebrates, are the following: 1st, the nerves of touch terminate in cells; 2d, these are situated in the skin, (both in the corium and the epidermis) and adjacent mucous membranes; 3d, they are diffused over the entire body; 4th, they are especially numerous in the active organs of touch, where they often aggregate in groups, frequently having the appearance of small, isolated organs, but do not otherwise present any anatomical peculiarities, distinguishing them from those found in other parts of the body; 5th, the corpuscles of Meissner and Krause are nothing but groups of similar cells.

In the next part, the author submits the communications hitherto made on the free termination of tactile nerves to a critical examination. In succession he enumerates all the results which have been obtained by the aid of the reduction of chloride of gold; he lays open to doubt the possibility of demonstrating the epithelial terminations of nerves on the recent cornea, and he concludes from this that the use of the gold methods does not lead to results worthy of reliance. This is the reason why the author rejects as problematical the observations hitherto made on the free terminations of the nerves of touch.

He arrives at the same results with regard to the closed terminal nets. He states that though reticular plexuses are formed by the division of a nerve trunk into fibrils, and the continual crossing of these, yet no true network is formed where the ultimate fibrils reunite, forming closed loops.

In conclusion, he dwells on some terminations of nerves, the nature of which as nerves of touch is yet undetermined, viz.: 1st, the Pacinian corpuscles in man and the other mammals for which he refers to the publications of Axel Key and Retzius, which were published in this Archive, Vol. IV., No. 25; 2d, the *organs of the sixth sense* of Leydig. (*Ueber Organe eines sechsten Sinnes. Nov. Act. Acad. Cæs. Leop. XXXIV.*, 1868, and *Archiv. f. mikr. Anat.* XII., 1875).

These organs are not, according to the author, organs of a special sensation, as Leydig will have it, but they are partly terminations of the gustatory nerves and partly of the nerves of touch. As to the lateral organs of fishes and tadpoles, he is yet in doubt to which of the two above mentioned categories they should belong; but he feels assured that there is not the slightest reason of probability for admitting them as organs of a sixth sense.

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THE FUNCTIONS OF THE LIVER.—Many of our readers have doubtless perused the rather remarkable paper by Dr. Lautenbach, published not long since in the *Phil. Med. Times*, on certain new functions of the liver.

He reports the experiments as having been performed in the laboratory of Professor Schiff, at Geneva. The experiments were exceedingly suggestive, and their publication marked their author as one of the most promising and active of our younger physiologists. But it has been with some surprise, to say the least, that we find in No. 36 (1877) of the *Centralblatt f. d. med. Wissenschaften*, the following note, in italics, from Prof. Schiff:

"In No. 32 of the *Centralblatt*, there is reported a series of experiments on 'The Functions of the Liver,' with the statement that these experiments were, for the most part, performed in my laboratory. The greater part of these experiments, more than four hundred in number, were not only performed in my laboratory, but were performed by myself, and in fact, for the completion of a work previously undertaken, and I have already communicated an outline of them to the Societ  de Physique, and printed it in the March number of the *Archives*. Herr Dr. Lautenbach, of Philadelphia, who was then engaged on other work in my laboratory, made notes on a part of my experiments, as other visiting students and not merely those actually working here were permitted to do. When I had finished the series on nicotine, Herr L. recommended that I should repeat my experiments upon hyoscyamine; and I told him to take them in hand himself, so far as they were performed on frogs, and to follow out thoroughly the method I had used with the nicotine. This, as I have stated in the *Archives*, was the only active participation of Herr Lautenbach in my researches. Later, I permitted him, at his request, to publish in English an outline of these experiments, together with my verbal statements as to similar researches undertaken before his coming to Geneva. I allowed him to use for this purpose a series of notes; but was in the highest degree astonished to see my whole investigation, garnished with some plainly mistaken statements of Lautenbach's (for example, the statements in regard to the portal system of frogs), and with some independent investigations with coniin and cobra poison added, appear under *his* name and as the product of *his* labor. Herr L. has, in previous numbers of Philadelphia medical journals, converted other practical laboratory demonstrations to his own use. If such things are allowed to go unresented, practical laboratory demonstrations and independent investigations by students are threatened with serious peril."

The only comment we feel inclined to make on this case, is, that we hope Dr. Lautenbach may be able to explain the allusions, contained in Dr. Schiff's note, in some satisfactory manner.

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The following are among the articles lately published on the Anatomy and Physiology of the Nervous System.

ROSENBACH, On the Physiology of the Vagus, *Centralblatt*, No. 6; BENEDIKT, The Occipital Lobes in Mammals, *Ibid*, No. 10; TCHIRIEW, Irritability of Nerves and Muscles, *Ibid*, No. 21; PANSCH, Some Points in Regard to the Cerebral Convolutions, *Ibid*, No. 36; BULGAK, On the Contraction and Innervation of the Spleen, *Virchow's Archiv.*, LXIX., ii.; PFLUEGER, Remarks on the Physiology of the Central Nervous System,

*Pflügers Archiv.*, Bd. xv., iv. and v. Hft.; LANGENDORFF, On Reflex Inhibition, *Archiv. f. Anat. u. Phys.*, Phys. Abth., i. and ii., 1877; KLUG, Physiology of the Space-Sense of the Upper Extremity, *Ibid.*, ii.; HERMANN, Researches on the Development of the Muscle-Current, *Pflügers Archiv.*, xv., iv. and v.; BERNSTEIN, On Fatigue and Recovery of Nerves, *Ibid.*, xv., vi. and vii.

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## b.—PATHOLOGY OF THE NERVOUS SYSTEM AND MIND, AND PATHOLOGICAL ANATOMY.

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NERVOUS SYMPTOMS WITH EAR DISEASE.—Dr. Hughlings Jackson, *Lancet*, (Am. Rep'r.) June, considers the following nervous affections occurring in connection with ear disease; neuralgic pain, Bell's paralysis, tumor of brain, abscess and meningitis, hemiplegia, epilepsy and epileptiform seizures, and aural vertigo. Some of these, as he says, can only be considered as associated with aural trouble, their relations are not otherwise clear.

Neuralgic pain, he thought, was probably only symptomatic of exacerbation of the tympanic disease, and, *as a rule*, did not precede meningitis or abscess, but sometimes preceding paralysis of the face, it may lead to a false diagnosis of "rheumatic" paralysis of the portio dura nerve.

The following remarks were made on unilateral facial paralysis: (a) He had never met with paralysis of the palate in uncomplicated facial palsy. An obliquity of the uvula indicated nothing, since it might occur in healthy individuals. If paralysis of the palate was met with he should suspect not a dependence upon ear disease, but intracranial trouble. There would be two lesions, and if the symptoms came on slowly he should suspect syphilis. (b) Uncomplicated facial paralysis with ear disease is not an intracranial or cerebral, and hardly an aural symptom; it is a bone symptom. (c) It is occasionally the precursor of serious intracranial trouble, but not often, and their relations are only those of coincidence. (d) It is erroneous to infer that because the causal lesion of the palsy is a gross one, that recovery will necessarily not occur. (e) The facial palsy does not even indicate serious extension of the ear disease, but only extension in an unfortunate direction. The occurrence of intense pain in the head would be a much more serious symptom.

In some cases of chronic ear disease he had found a mass of tubercle in the cerebrum or cerebellum in the place, so to speak of an abscess from aural disease. Except, perhaps, in chronicity, there are no distinguishing symptoms between the two.

Meningitis and abscess were mentioned together on account of the difficulty in diagnosing one from the other. Abscess was much the most frequent, however, and therefore the safer to predict. Perhaps the very early appearance of optic neuritis, if well marked, would point to abscess. An abscess, as is well known, may give rise to no symptoms, and when then they do come on the case is usually acute, and may simulate meningitis. Some cases of tumor have very similar symptoms, the pres-